

primary beam, and a maximum in, or opposite to, that direction.

On the neutral pair hypothesis, if we only assume that the chance of ejection from an atom is equal in all directions in the plane of rotation, it may as simply be shown that the directions of minimum and maximum intensity are the same as on the previous hypothesis; but whereas on the ether pulse theory the intensity of the secondary rays in the direction of propagation of the primary is double that in a direction at right angles, on the "neutral pair" hypothesis it varies as the cosecant of the angle which the direction of propagation of the secondary makes with that of the primary, becoming infinite along the direction of propagation of the primary. In other words, the intensity of secondary radiation varies as the density of the lines of longitude (or as the secant of the latitude) on a sphere with the secondary radiating mass at its centre, the direction of primary propagation being along the axis.

I have made experiments to test the two hypotheses, using an electroscope to compare the intensities of secondary radiation as nearly as possible in these two directions. Taking into account the finite section of the beams and consequent obliquity of the rays, the ratios on the two hypotheses would be roughly 1.9:1 and 8:1, assuming perfect scattering and neglecting the effect of tertiary rays in the first case and assuming the plane of rotation to contain accurately the direction of propagation in the second. If the assumption is only approximately correct in either case, the ratio will be somewhat reduced. It is evident that great accuracy in the experiments was not essential. They, however, leave no doubt as to the conclusion, for the ratio of intensities was roughly 1.6:1—one that might be expected on the ether pulse theory, and appears impossible on the other. It is possible that with suitable primary rays and thickness of secondary radiator, results showing more perfect scattering will be obtained.

These preliminary experiments, however, to my mind furnish quite conclusive evidence in favour of the ether pulse theory.

CHARLES G. BARKLA.

University of Liverpool, October 26.

On Correlation and the Methods of Modern Statistics.

I do not know that much profit is likely to arise from continuing this discussion further; it appears to me to be merely unwrapping considerable convolutions in Mr. Hinks's mental attitude towards Miss Gibson and myself. The chief charge made at the British Association was that we had overlooked a curved regression line between magnitude and parallax—that now appears to have disappeared into limbo. In his first letter to NATURE Mr. Hinks apparently objected to our finding "a quite significant and important" relation between parallax and proper motion, but one not more than half-way up the correlation scale. He has now discovered that "the point of most general interest" is that of colour. He charged us with stating a far-reaching suggestion on the basis of the Cape stars. It turns out now that the element in our far-reaching suggestion is not the *suggestion* at all, but what I am prepared to assert as a fact, namely, that the magnitude of the stars "is not mainly determined by parallax or distance, but is more closely associated with colour, and thus probably with chemical or physical condition." The colour and magnitude correlation is essentially that determined by Miss Gibson, 0.3; the values for the spectral class and magnitude correlations run up according to the classification used to double this value, and even to 0.7. The colour and spectral class correlations reach, as we might expect, a still higher value. Meanwhile, the magnitude and parallax relation in its best determination is 0.28. I agree with Mr. Hinks that this is a point of "general interest," and I am glad that his last letter enables me to assert it, not as "the vaguest of suggestions," which words had reference to the discontinuity of frequency in star counts, but as a fact which may be slightly modified when more data are reduced, but is substantially correct as I have given it.

KARL PEARSON.

The Interpretation of Mendelian Phenomena.

I AM sorry Mr. Lock should mistake what I devoutly hope is a sense of proportion for a desire to belittle Mendelian work. In science clear ideas are of importance, and I wished to elicit something more definite than the vague notion that Mendelism will someday and somehow furnish a master key to the problems of heredity. I made no complaint that Mendelism "does not immediately lead to the solution of all the most difficult problems which biology affords," as Mr. Lock rather extravagantly asserts, but merely asked what conceivable bearing it can have on any problem save that of sex. By the problem of sex I mean the problem of the function of sex—or of conjugation if Mr. Lock prefers. I confess I cannot imagine what light Mendelism has shed on the question of the alleged transmission of acquirements, and as for the "problems of the actual transmission of characters," these, as dealt with by Mendelians, are nothing other than problems of sex. That is, Mendelian experiments demonstrate nothing more than the degree in which certain characters (mutations) are transmitted or distributed under, or affected by, conditions of conjugation. Doubtless it is true that the majority of Mendelian cases have been observed in self-fertilised types, but I am not aware that they have ever been observed unless cross-fertilisation had previously occurred. In parthenogenesis the individual arises from an unfertilised ovum; how, then, is segregation possible? What segregates?

The evidence on which I base my assertion that there is no segregation in the mulatto is that of my own eyes. Mulattoes vary amongst themselves, but the blend is usually very obvious, and is reproduced in subsequent generations when breeding is *inter se*. With every infusion of European blood the negro type—skin colour, hair texture, shape of features, and the like—grows fainter, until at length the "touch of the tar-brush" is hardly if at all perceptible; and this blending, so far as I am aware, occurs, not only in all crossed human varieties, but in other natural varieties as well. There may be exceptions; in fact, I believe there are; but blending appears to be the rule in the vast majority of instances.

How can the fact that human races have crossed more often than any other animal complicate the problem? My statement implied, not that every human race is a chaotic mixture of types, nor even that there are no pure types, but only that we have here a very large and varied mass of material on which to found our judgments. Nor did I imply that mutations are especially frequent under conditions of cultivation. I believe they are quite as common in nature. Our hospitals and asylums are full of them—hare-lips, cleft-palates, club-feet, hæmophilia, colour-blindness, deaf-mutism, feeble-mindedness, and so forth. Their inheritance is usually Mendelian, but I never heard of a human mutation that was useful. I implied merely that artificial selection is founded on mutations, and that the striking difference between artificial and natural varieties indicates that natural selection is not founded on them. We know the past and present of man better than that of any other type, certainly of any natural type. Men are fond of noting wonders, and we have a written history of thousands of years; but never yet has the differentiation of a human variety by mutation been recorded. On the other hand, so surely as a human race separates into sections, between which there is little or no intercourse, gradual differentiation sets in, which, under conditions of savage warfare and very restricted intercourse, may be seen in the inhabitants of quite small tracts of country, as in New Guinea. Amongst plants and lower animals parthenogenetic types are particularly rich in varieties. "Thousands of forms may be cultivated side by side in the Botanical gardens and exhibit slight but undoubted differentiating features, and reproduce themselves truly by seed" (de Vries, "Species and Varieties," pp. 59-60). When reproduction is bi-parental, varieties are few if individuals from distant parts of a wide area are able to mate, and proportionately more numerous if intercourse is more restricted. Thus in every valley of Samoa is found a distinct variety of snails; but species of birds, mammals, and fishes which possess considerable powers of locomotion have few varieties. Is Mr. Lock able to conceive any

interpretation of all these facts except that under natural conditions fluctuations are selected and inheritance is blended?

He lays stress on the circumstance that man is not amenable to experiment; but man is not the only species that has natural varieties. May I, in turn, lay stress on the fact that it seldom pays the cultivator to select small differences (fluctuations)? Of necessity he selects mutations. The Mendelian experimenter has practically limited himself to the materials so created. He himself chooses for his experiments, and can choose, only glaring differences. In other words, he has, *qua* experimenter, absolutely no acquaintance with the small differences (fluctuations) which normally distinguish mating individuals in natural breeding. He judges the normal from the abnormal, the rule from the exception, and then appeals to earth to note the precision of his methods and thanks heaven he is not as other men, even as mere observers who seek to take the whole of the facts into consideration. Notwithstanding his parade of exactness, his belief that he reproduces natural conditions "is an assumption which still lacks the support of facts." Once more, therefore, let me challenge Mr. Lock and his comrades. If Mendelism deals with any other problem but that of sex, what is that problem? If no other problem can be named, what is the evidence that Mendelism deals with anything more than those abnormalities of sexual reproduction which occur under conditions of artificial selection? As I say, I do not ask for the solution of any problem. I ask only for an indication that Mendelism has any conceivable bearing on it. If the latter question also cannot be answered, then by all means let Mendelians pursue their very interesting studies; but let it be understood that "the new science of genetics" implies, not the study of heredity in general, but only the study of certain curiosities of artificial breeding.

G. ARCHDALL REID.

Southsea, October 20.

Pagan Survivals and Christian Adaptations.

It may interest some of the readers of NATURE to find that the institution of the "kern-baby" (corn-baby) still exists in our island; and a writer in the *Christian World* for October 3 was present at the bringing home, on the last load, of this Pagan institution, and was present at the harvest supper this year, when the effigy was honoured by being placed on the table. It was, presumably, only a survival of olden time, when our ancestors "ate and drank" with their gods—especially the gods of agriculture (Judges, 9, v. 27).

Again, I received a letter the other day from the rector of Fobbing, Essex (formerly rector in the Scilly Islands), informing me, in reply to an inquiry, that the Beltane fires are, up to the present day, lit there on the highest point of the islands on May eve, just as our ancestors lit them in honour of the rise of Baal (or the sun). My informant, who has only left the islands two years, often witnessed the jumping of the youths "through the fire." I should be very pleased to learn of any ancient customs of this kind still carried out on the eves of "May Day," "Easter," "All Hallows," "Christmas," or other solstitial and equinoctial periods, and not heretofore recorded in standard books on the subject. In trying to ascertain the uses of certain stone circles and monster cromlechs this evidence is of great importance, as the early missionaries purposely "adapted" so many of the Pagan festivals to Christian worship. Wales is the most promising field.

J. W. HAYES.

West Thurrock Vicarage, Grays, Essex, October 16.

The "Quaternary."

IN reply to Dr. Wright's comment on my letter (p. 639), I would point out that the restricted use of the word "Quaternary" appears to be confined to anthropologists. Geologists (Sir Archibald Geikie, Prof. Kayser, and Prof. Lapworth, for instance) who employ the term include in it everything from the commencement of the Glacial period to the present time.

JOHN W. EVANS.

Imperial Institute, October 25.

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THE "MAURETANIA."

THE first impression of the *Mauretania* is one of colossal size, the last is wondering amazement at the forethought and design which appear in details, trivial in themselves, but of supreme importance to individual comfort, of the fittings. Only those who saw the ship in the narrow waters of the Tyne can realise her huge dimensions. Eight hundred feet long herself, she floated abreast the builders' yard in a river less than 900 feet wide, which runs in a narrow cleft between low hills. In that narrow valley the great bulk of the ship made a prodigious spectacle, and over the valley before the start on the maiden voyage the smoke from her four great funnels moved like a pall.

In the brief voyage from the Tyne to the Mersey which took place last week, some of the peculiar features of the great ship were revealed. The Tyne is winding and narrow, and on the Tuesday afternoon its course was obstructed by crowds of steamers laden with sightseers. In this difficult passage the handiness of the vessel was at once apparent. Proceeding under her own steam, steered by propellers and the rudder, she was easily manœuvred at the sharp bends. To the writer, who was on the bridge at the time, it was obvious that the great turbines, which in the aggregate can develop 70,000 h.p., can be stopped or started with ease and certainty.

At sea, though the recurrent shocks characteristic of vessels fitted with reciprocating engines are absent, vibration is noticeable, though relatively slight. Generally speaking, it is maximal in the after-part and diminishes thence to the bows. The distribution, however, is erratic, regions of maximal vibration often being close to regions of minimal vibration. In the great dining saloon at 22 knots the tremors were barely noticeable, being something like the passage of a vehicle in a street outside. On the other hand, a region of marked vibration was forward of this, about the level of the second funnel.

The cause of the vibration in turbine-propelled ships is not at all obvious, and experts at present seem to be unprovided with a satisfactory hypothesis. The turbines themselves are singularly free from it. Leaning against their great steel shells one is not conscious of a movement. In the shaft tunnels, however, it is very marked. The vibration has been referred to the impact of the water thrown by the blades of the wing propellers against the sides of the ship, to the unequal thrusts which each blade exerts in the course of each revolution, and to the formation of twisting couples between the propellers when they synchronise in certain ways. Inequalities in the thrust arise from the fact that owing to skin friction the water near the side of the ship is dragged bodily along with it. Each blade, therefore, as it revolves, passes through water moving in the direction of the ship to water which, relatively speaking, is still.

The vibrations themselves are markedly periodic, mounting by a long crescendo to a climax, followed usually by complete quiet. This periodic nature unquestionably suggests a dependence upon synchronism between propellers on opposite sides of the ship, and it was found in the case of, I believe, the *Deutschland* that vibration was much lessened when her twin screws were set to rotate respectively at 70 and 80 times a minute instead of both being at approximately the same rate. The whole subject is being investigated on the *Mauretania* by means of the pallograph, which registers at the same time the shaft movements and the vibrations.

As it is not possible to get an indicator diagram of a turbine, the work done is measured on the